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ON THE NECESSITY OF ATOMIC THEORIES IN PHYSICS.[1]

IN addition to the atomic hypothesis now in vogue, there is a second method employed in theoretical physics which seeks to represent the facts of some given and very narrowly circumscribed domain by means of differential equations. We shall call it *mathematico-physical phenomenology*. Since this method offers a new constructive representation of the facts, and since it is of advantage to possess as many such representations as possible, unquestionably the new doctrine is qualified to take rank beside the atomistic hypothesis in its present form as a method of great value and utility. Another form of phenomenology, which I should like to term the *energic phenomenology*, will claim our consideration later.

Now, the opinion has grown rife that our representations of nature as drawn from phenomenological considerations enjoy intrinsic advantages over our representations as obtained by the atomistic method. It is my wont to avoid general philosophical discussions of this character, where there are no practical consequences at stake, for the reason that they do not admit of the precise formulation that special questions do, and consequently their solution remains largely a matter of taste. But I have the impression that the doctrine of atomism has, for the rather untenable reason above adduced, recently been fast losing ground, and I have therefore felt constrained to do what is in my power to avert the injury that I believe would result to science if phenomenology should be made a dogma, as atomism once was.

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[1] Translated by T. J. McCormack from the *Proceedings of the Imperial Academy of Sciences of Vienna*, Vol. CV., No. 8, Section of Mathematics, Astronomy, Physics.

To avoid misunderstandings, I shall declare it at the outset to be the purpose of the following discussion to answer certain quite definite questions. And since the services that atomism has performed for science cannot possibly be a subject of doubt, these questions may be formulated as follows:

(1) Has not the atomic theory in its present shape distinct advantages over the phenomenologic theory now current?

(2) Is there any likelihood that there shall develop from phenomenology in the near future a theory possessing precisely the advantages which constitute the distinctive features of the atomistic theory?

(3) Granting the possibility of the atomic theory's being eventually discarded, does not the equal possibility exist that gradually phenomenology may become absorbed in the atomic theory?

(4) Finally, would it not be a positive detriment to science, if the reigning views of atomism did not continue to be cultivated with the same ardor as the views of phenomenology?

I may state at once that the outcome of the following reflexions will be to answer the foregoing questions in favor of atomism.

The differential equations of mathematico-physical phenomenology are obviously nothing but rules or directions for constructing and combining numbers and geometric concepts. But these numbers and concepts are themselves nothing but mental constructs from which the phenomena of nature may be predicted;[1] and such is precisely the case also with the constructs of atomism, so that for my part I can discover not the slightest difference between them in this regard. Indeed, to my mind and feeling no direct description of a comprehensive domain of facts, no single and exclusive mental representation of it, at all, is possible. It is accordingly wrong to say with Ostwald, "Thou shalt make unto thee no mental image or likeness whatsoever," but one may merely say, "Thou shalt give to such images the fewest possible arbitrary features."

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[1] Compare Mach's *Principien der Wärmelehre*, Leipsic, J. A. Barth, 1896, p. 363. Mach's writings have contributed much toward shaping my philosophy.

Mathematico-physical phenomenology often combines with its preference for the differential equations a certain disparagement of atomism. But it is my conviction that there is a vicious circle running through the reasoning that the differential equations transcend the facts in less degree than the most generalised form of the atomistic conception. Of course, if we are antecedently of the opinion that our sense-perceptions are mentally representable by the simile of a *continuum*, the differential equations will not, and the atomistic conceptions will, transcend our preconceived opinion. But quite different the case if we are wont to think atomistically: then the situation is reversed, and the conception of a *continuum* appears to transcend the facts.

Let us analyse, for example, the import of Fourier's equation for the conduction of heat, which has become classic in this regard. This equation is merely the statement of a rule, consisting of two parts:

(1) Conceive in the interior of a body (or, more generally, regularly aranged in some correspondingly bounded three-dimensional manifold) a large number of tiny things (call them elementary particles), each of which at the outset has any temperature you please. After the lapse of a very small interval of time (or on a very slight increase of a fourth variable), let the temperature of each particle be the arithmetic mean of the temperatures which the immediately surrounding bodies previously had.[1] After a like interval of time, repeat this process; and so on.

(2) Conceive both the elementary particles and the minute intervals of time to diminish constantly in size and their number to increase in a corresponding ratio until thermometric results are reached of sufficient accuracy to render the influence of further diminution inappreciable.

Likewise, the definite integrals that give the solution of the differential equations, can in general be evaluated only by mechanical quadratures, or, in other words, themselves first require decomposition into a finite number of parts.

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[1] Mach, *loc. cit.*, p. 118.

Let no one fancy he has attained a clear comprehension of what a continuum is from the mere utterance of the word *continuum* or merely by writing down a differential equation. On closer scrutiny, the differential equation will be found to be merely a statement of the fact that at the outset a finite number of things is to be held in the mind; this is the condition precedent of the entire process; not until afterwards is the number of the things increased until further increase is without influence on the result. Of what advantage is it now to suppress the postulate requiring us to conceive large numbers of individual things just after we have in our explanation of a differential equation defined by means of that postulate the value expressed by that equation? I may be pardoned the rather hackneyed expression if I say that the person who imagines he has cast off the thrall of atomism by differential equations does not see the woods for the trees. And as for explaining differential equations by complicated geometrical or other physical notions, this would be an out-and-out presentation of the subject in the light of an analogy, instead of a direct description. In point of fact, we are unable to distinguish the adjacent parts. But a constructive image in which we were unable to distinguish the adjacent parts at the very beginning, would be indistinct; we could not perform the prescribed numerical operations with it.

In declaring, therefore, a differential equation, or a formula containing definite integrals, to be the most appropriate representation of the facts, I am surrendering myself to an illusion if I fancy that in so doing I have eliminated the atomistic conception from my mental representation, without which conception the notion of limit is meaningless; I am rather merely making the additional assertion that to whatever degree of refinement our means of observation may be pushed, there will be no noticeable differences ever discovered between the limiting values and the facts.

Does not that constructive representation which assumes a very large but finite number of elementary particles, transcend the facts far less than such an implication? Is not the situation completely reversed? Whereas formerly the assumption of atoms of a definite size was regarded as a crude and arbitrary theory passing unnecessarily beyond the facts, now that assumption appears the more natural one, and the assertion that there will never be differences discovered between the facts and the limits for the reason that such have never hitherto been discovered (perhaps not once in all cases), adds to the representation something new and undemonstrated. Why the representation is made clearer, or more probable, by this adscititious and supplemental assertion is incomprehensible to me.[1] Atomism appears to be inseparable from the notion of a continuum. Laplace, Poisson, Cauchy, and the rest evidently proceeded from atomistic conceptions for the reason that inquirers were at that time more perfectly aware of the fact that differential equations were only symbols for atomistic conceptions, and that they therefore felt more vividly the need of simplifying these conceptions.

Like the equation for the conduction of heat, the fundamental equation of elasticity can in general be solved only by first conceiving a finite number of elementary particles which act on one another according to certain simple laws, and by subsequently seeking the limit after augmenting the number. This limit is thus again the real definition of the fundamental equations, and that representation of the facts which assumes at the outset a large but finite number of things, again appears the simpler.

We may obtain thus, by investing the atoms in question with as many properties as are necessary to describe a small province of facts in the simplest manner, a special atomistic system for every province of facts,[2] which, whilst it is in my opinion as little a direct description as that which is commonly called atomism, is yet a representation of the facts as devoid as can be of adscititious elements.

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[1] Our sensations of sight correspond to the excitations of a finite number of nerve-fibers, and are thence doubtless better represented by a mosaic than by a continuous surface. The same holds true of the other sensations. Is it not more probable, therefore, that the models representing complexes of sensations are more fitly composed of discrete parts?

[2] If Hertz is consistent, he can assign to his statement that a definite system of differential equations constitutes his theory of electro-magnetic phenomena, no other meaning than that he thinks these phenomena under the image of two heterogeneous intellectual entities filling space and having both the characteristics of vectors, the variation of which in time, here with reference to intensity and direc-

Now, phenomenology endeavors to combine all these atomistic constructs without previously simplifying them, in order to de-

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tion, is, as in the conduction of heat, determined solely by the immediate environment, but is dependent on the same in a more complicated though readily assignable manner. We have given in this an atomistic theory of electro-magnetism, which contains as few adscititious elements as possible. The demand that this domain shall be explained mechanically, is but another expression of the need of simplifying the construct representing it and of rendering it homogeneous with the constructs representing the facts of the other departments. It is to this lack of homogeneity, which does not impress one on merely comparing the differential equations superficially, and to the probability that simpler constructs exist, that people doubtless wish to give expression when they say we do not know what electricity is.

The ordinary equations of the theory of elasticity, when additionally involving the displacements u, v, w, and the elastic forces X_x, $X_y \dots$, represent (if the meaning of the notion of limits be recalled) pretty complicated rules for the variation of the coördinates $x+u$, $y+v$, $z+w$ of ordinary points, and the simultaneous variation of vector-atoms. Even the equations which arise after elimination of the elastic forces, are in need of some reduction before they yield the customary atomistic construct of elastic phenomena. To obtain the latter, certain combinations and decompositions of the equations or of the constructs representing them have to be performed, just as in mechanics forces have to be composed and decomposed in order to obtain as simple a description as possible.

Likewise, differential coefficients with respect to time impliedly require that in our mental construct of nature time shall be conceived at the outset as decomposed into very small, finite portions (time-atoms). If I relinquished, therefore, as unsubstantiated by experience, the view that deviations can never be discovered from the limit to which the construct approaches as the time-atoms are diminished in magnitude, I should be forced to the contention that even the laws of the mechanics of a material point are only approximately correct. Just to give some slight idea of the variety of the constructs at our command, I shall sketch forth here one special example of the representations in question.

Conceive in space (or better, in a tridimensional manifold) a large number of spheres in contact. The arrangement of these spheres varies, in accordance with a law A still to be determined, very slightly from one time-atom to another, but nevertheless by a finite amount. The variously formed interstices between the spheres take the place of the atoms of the old construct, and the law A is to be so chosen that the variations of the interstices in time shall furnish a theory of the universe. If it were possible to discover such a construct, which exhibited more homogeneity than the usual atomism, its title would be established by this fact alone. The conception of atoms as material points, and of forces as functions of their distance, is thus in all likelihood a provisional view, which for lack of a better must still be retained.

scribe the facts as they actually are, that is, to bring into conformity with them all the conceptions contained in these various atomistic theories. But, forasmuch as they introduce a multitude of notions gathered from many narrow provinces of facts, not to speak of a host of differential equations of which each, despite varied analogies, furnishes its own quota of idiosyncracy, it is to be antecedently expected that the description will assume an extremely complicated form. In point of fact, it appears that enormously complicated equations, utterly lacking in synoptic features, are requisite even when phenomenology attempts to describe the concatenation of some very few provinces of phenomena where the processes are virtually stationary (say, elastic deformation accompanied with heating and magnetisation, etc.). Furthermore, hypothetical features, features transcending the facts, must perforce be introduced when it is attempted to describe, for example, the dissociation of gases with Gibbs, or that of electrolysis with Planck.

Then there is the additional consideration that all the concepts of phenomenology have been derived from approximately stationary phenomena, and will not hold for turbulent motions. For example, we may define the temperature of a quiescent body by means of a thermometer inserted in it. If the body move as a whole, the thermometer may move with it. But if every element of volume of the body have a different motion, the definition is rendered nugatory, and it is then probable, or rather possible, that the different forms of energy (as to what is heat and what visible motion) can no longer be sharply distinguished.

If this be borne in mind, and account be also taken of the complications which the phenomenological equations assume even in the few instances where the concatenation of several provinces of phenomena is to be described, we shall obtain some conception of the difficulties encompassing the description by this method of turbulent phenomena in general, especially such as are accompanied with chemical changes, that is to say, of describing them without previously harmonising by arbitrary simplifications the various atomistic theories appertaining to the several provinces.

A special phenomenology, which I shall denominate the *energic*, in its widest acceptation seeks to bring some sort of harmony into the atomistic theories appertaining to the different fields, by ascertaining what features are common to them. Two classes of such features are known. To the first class belong certain general theorems, like the conservation of energy, the principle of entropy, etc., general integral theorems as I might term them, which hold good in all provinces. The second class consists of analogies which pervade a great variety of provinces. These are largely nothing more than a similarity of form which certain equations always assume at a certain stage of approximation, whilst in more minute details the analogies would frequently appear to fail.[1] Yet, in spite of the enormous importance of the integral theorems, which is owing to their universal validity and the high degree of certainty that flows therefrom, and in spite of the importance of the analogies, which is due to the advantages in computation and the new points of view they offer, both integral theorems and analogies furnish a small fraction only of the entire range and compass of the facts. Inquirers were compelled, therefore, in order even to describe exactly single domains, to introduce additionally so many special constructs (the natural history of the domain), that they could not obtain, as I believe I have shown elsewhere, a single unequivocal and comprehensive description of any one department of stationary phenomena by this method, let alone a synoptic view of all, or, worst of all, of turbulent, phenomena. The question whether comprehensive representations of nature will ever be obtained along this line, has therefore a purely academic value.

To reach this latter goal, the current atomistic theory is seeking to bring the fundamental features of the various phenomenological atomisms into better conformity with one another, by arbitrarily extending and curtailing the properties of the atoms required in each province, and thus rendering them suitable for the simultaneous description of several provinces.[1] It decomposes, so to speak, the properties of the atoms required for a single province, into components (see p. 70, footnote), rendering them applicable to several provinces. Naturally, as in the case of the decomposition of forces into components, this is not possible without some violence and transcension of the facts.[2] In compensation, however, it enjoys the advantage of offering a simple and synoptic representation of a far greater mass of facts.

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[1] Approximate proportionality of slight changes of the function to changes of the argument, the remaining of the first and second differential quotients with approximately constant coefficients, linearity with respect to small quantities and consequently superposition. So, too, the analogies in the behavior of the different forms of energy appear in part to have such purely algebraical sources.

Whereas phenomenology stands in need of distinct and largely unallied constructs even for the mechanics of the movements of the centre of gravity and of rigid bodies, for elasticity, hydrodynamics, etc., the present atomic theory forms a perfectly apposite representation of all mechanical phenomena, and in view of the compactness of this province it is hardly to be expected that phenomena will yet be discovered that do not fit into the frame of the system. The atomic theory also embraces the phenomena of heat.

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[1] I do not mean to say by this that the phenomenologic equations have always preceded the advances made by modern atomism in point of time. As a fact, most of the phenomenologic equations were reached by the consideration of specialised forms of atoms taken from another province (mechanics), and did not acquire until subsequently, by being stripped of these considerations, the character of phenomenologic equations. This circumstance cannot surprise us when it is known that these equations in reality always involve the supposition of atomistic constructs, and it can therefore only tell in favor of atomism.

[2] One property of this sort arbitrarily introduced into our conception of atoms is their unchangeability. The criticism that we are concerned here with an unwarranted generalisation of the merely transitory unchangeableness of solid bodies, would be justified if we attempted, as was formerly done, to prove the atoms unalterable, *à priori*. We have introduced this property into our conception in order to render it capable of representing in aggregate as many phenomena as possible, just as we introduce into the equation for the conduction of heat the first differential coefficient with respect to the coördinates, in order that it may conform to the facts. We are ready to relinquish unalterability in all cases where another assumption would more fitly represent the facts. In reality, the vector-atoms of the ether mentioned in the footnote on page 70 would not be unalterable with the time.

The unalterability of the atoms belongs thus to those conceptions which have proved themselves of great service, although the metaphysical considerations by which they have been reached cannot stand before unbiassed criticism. But it is precisely owing to this serviceability that we are forced to grant at least the probability that radiant energy so called admits of representations by means of constructs similar to matter (that the luminous ether is a substance).

The fact that this latter circumstance cannot be definitely demonstrated, is due to the difficulty of calculating the molecular motions. In any event, all essential facts may be rediscovered in our theory, which has also proved itself extremely useful in the presentation of crystallographic facts, of the constant proportions of masses in chemical combinations,[1] of chemical isomerisms, of the relations between rotation of the plane of polarisation and chemical constitution, etc., etc.

Then, again, the atomic theory is susceptible of still greater development. We are at liberty to conceive among the atoms individuals endowed with any sort of properties, as, for instance, the vector-atoms, which, as we saw in the footnote on page 70, yielded for the time being the simplest description of electromagnetic phenomena.[2]

As to the turbulent phenomena which are as yet entirely inaccessible to phenomenology, the attitude of atomism toward these is admittedly one of definite preconceptions; yet in compensation for this bias, it offers valuable clues for the most likely modes of presentation of the phenomena, nay in many cases even enables us to predict them. Thus the theory of gases is able to predict the course of all mechanical and thermal phenomena in gases, even in the case of turbulent motions, and so affords indications as to how temperature, pressure, etc., are to be defined for these phenomena. Now this is precisely the chief end and purpose of science, so to shape the constructs representing one group of facts that from them may be predicted the behavior of other similar facts. It is understood, of course, that the prediction is afterwards to be checked by experiment. Probably it will be only in part confirmed. There is then hope of so modifying and supplementing the constructs that they will conform also to the new facts; which is tantamount to making new discoveries with respect to the constitution of the atoms.

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[1] No chemical combination is produced instantaneously; each is propagated in space with a finite, though great, velocity. Applying therefore the analysis of the notion of continuity above presented, Mach's and Ostwald's theory of chemism will be found to assert that in each instance a elementary particles of one substance and b of another disappear, while c of a new substance make their appearance. The difference between this and the current conceptions of chemistry is manifestly not very essential. Nothing material would be altered if the limit as usually found should be made to represent the facts.

[2] If by a mechanical explanation of nature be understood one reposing on the present laws of mechanics, it must be declared to be altogether problematical whether the atomic theory of the future will be a mechanical explanation in the sense defined. Only in so far as it shall be under the constant necessity of supplying the simplest possible laws for the time-variation of numerous individual entities distributed in a manifold of probably three dimensions, may it be termed, at least in the traditional sense, a mechanical theory. For example, should no simpler description of electromagnetic phenomena be actually forthcoming, then the vector-atoms mentioned in the text would have to be retained. Whether the laws according to which these vary with the time are to be termed mechanical or not, depends wholly on our own taste.

Unquestionably, the demand is justified that no more adscititious elements (and even these are to be as general as possible) shall be added to the construct than are absolutely necessary to describe extensive provinces of phenomena, that we shall always be ready to modify the construct, nay, even to bear in mind the possibility that it might be advisable to substitute an entirely new and utterly different one in its place. And for the very reason that the construction of the new construct would have to be effected on the basis of the old special phenomenologic constructs that have remained intact, therefore these also are to be carefully cultivated by the side of atomism.

In conclusion, I am inclined to go so far even as to assert that it is of the very nature of a construct, that it should introduce certain arbitrary features in its attempts at representation, and that strictly speaking one transcends experience every time one infers from a construct conforming to a given set of facts a single new additional fact. It is mathematically certain that, in order to represent all the facts, we should not be permitted to put in place of Fourier's equation for the conduction of heat an entirely different equation, identical with Fourier's only in regard to facts previously observed, with the result that on the first new observation made we should have to alter our construct utterly and with it our entire conception of the interchange of heat among the smallest particles. All bodies hitherto investigated, for instance, might accidentally exhibit just that particular group of regularities with the absence of which Fourier's equation would be rendered nugatory.

Just as Fourier transferred to the smallest particles (atoms) the law of specific heat and the fact that the interchange of heat between bodies in contact is proportional to the difference of the temperatures, so the theory of gases transfers to these particles the general laws of mechanics and the fact that bodies in close contact displace one another, but at some distance do not do so. With these smallest particles we cannot, as we see, dispense, when considering bodies of any size.

So, too, the assumption that the same atoms suffice for the description of both the liquid and gaseous aggregate states, appears to me, in view of the continuity of these two states, to be well founded, and conforms admirably with the requirement of simplicity in our description of nature. Granting the reasonableness of the two foregoing assumptions, we cannot escape the conclusion that the atoms are impressed with relative motions invisible to the eye, which absorb visible kinetic energy and the perceptibility of which by certain nerves is certainly not improbable (special mechanical theory of heat); and, further, that in highly rarefied bodies they usually describe nearly rectilinear paths (kinetic theory of gases). The construct by which we represent mechanical phenomena would only be made more complicated, if not self-contradictory, by abandonment of these conclusions. The additional assumption that the molecular motions do not cease whilst the induced visible motions are transformed gradually into molecular motions, also conforms perfectly to established mechanical laws.

All the consequences of the special mechanical theory of heat, howsoever disparate be the domains to which they pertain, have been confirmed by experience; indeed, I may say that they have been attuned in their minutest details to the rhythmical pulsations of nature.[1]

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[1] Of many instances I will mention but the explanation of the three states of aggregation and their transitions from one to another; further, the agreement of the concept of entropy with the mathematical expression of the probability or disorder of a motion. The assertion that a moving system of very many minute bodies

Fourier's assumptions with regard to the conduction of heat are of course of so extremely simple a nature, and the facts calculable from them conformed so perfectly to verified observations, that the assertion of his assumption and equations not being absolutely trustworthy, as first approximations, appears rather of the nature of casuistry. But I do not find it at all strange that the requirements of the situation should be satisfied by such extremely simple and plausible assumptions, for the province is very arbitrarily limited; or that subsequently facts greatly different from the kind already verified should be derivable from them.

If there should ever be forthcoming so comprehensive a theory as the present atomism, one likewise which reposed on so distinct and unassailable a foundation as Fourier's theory of the conduction of heat, then indeed this were an ideal. Whether this result is to be attained by the subsequent combination of the previously unsimplified phenomenologic equations, or whether it is to be realised by the eventual asymptotic approach of current atomic conceptions to the plane of evidence of Fourier's theory by continual adaptation and constant confirmation by experience,—this appears to me to be as yet entirely undetermined.[1] For, though the existing ob-

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tends, apart from unobservably few exceptions, towards a condition for which an assignable mathematical expression measuring the probability of the state is a maximum, appears to me to be superior, after all, to the almost tautological assertion that the system tends towards the most stable condition. As for the rest, Mach is correct in his conjecture (*loc. cit.*, p. 381) that at the time I composed a popular lecture on this topic I was unacquainted with the works cited by him as treating of the tendency toward stability, all of which but one were published years after my lecture, and all of them after the original papers of which my lecture was but a popular presentation.

If the principle of energy were the only support of the special mechanical theory of heat, and the explanation of that principle its only purpose, then the theory would be superfluous after the principle had been fully explained. We saw, however, that there are many other reasons speaking in favor of it, and that it furnishes a constructive representation of many other phenomena.

The theory of the electric fluids was from the outset artificial after an entirely different manner, and had been recognised by many inquirers from the start as provisional.

[1] Considerable remoulding and adapting (cf. Mach, *loc. cit.*, p. 380) are still to be done in the case of both theories. Fourier's equations for the conduction of heat

$$\frac{du}{dt} = k\,\Delta u$$

servations going to show that molecular motions in liquids and gases may be observed directly, be not regarded as conclusive, yet the possibility of future conclusive observations cannot be gainsaid. It accordingly seems to me utterly beside the mark to assert positively that such constructive representations of nature as the special mechanical theory of heat or the atomic theory of chemism and crystallisation shall ever vanish perforce from science. The sole question that can be raised is whether the precipitate zeal exhibited in the cultivation of such constructs, or the excessive caution that recommends total abstinence from them, be of greater disadvantage to science.

The extent to which physics, chemistry, and crystallography have profited from atomism in point of palpableness and perspicuity, is well known; that it has also been a clog, and hence in many cases has appeared as superfluous ballast, especially at the time when it far less perfectly conformed to phenomena than at present, and was more impregnated with metaphysical considerations, shall not be denied. Synoptic control will not be lost, nor certitude in the least impaired, if we rigorously sunder the phenomenology of the results that have been established with the greatest possible certainty from the atomic hypotheses that serve the purposes of unification, and continue to cultivate each with equal zeal, as alike indispensable. But we should not lay exclusive emphasis on the advantages of phenomenology and assert that this latter doctrine is one day to supplant the atomism now current.

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is, if k be constant, unquestionably wrong. That with k variable it should necessarily take the form

$$h\frac{du}{dt}=\frac{d}{dx}\left(k\frac{du}{dx}\right)+\frac{d}{dy}\left(k\frac{du}{dy}\right)+\frac{d}{dz}\left(k\frac{du}{dz}\right)$$

has hardly been sufficiently confirmed by experience. And it does not represent at all the effect on the distribution of heat of the compressions and dilatations inseparably incident to non-stationary conduction, or the direct action of the heated volume-elements on other more remote elements in a diathermanous body by thermal radiation (and who knows but all bodies are diathermanous for certain rays transmitting energy and necessarily therefore also heat). If it be said these phenomena are not part of the process of pure thermal conduction, it may be replied that a pure process of this character is then nothing but a metaphysical hypostatised concept.

Even though the possibility exist of combining the constructs of phenomenology into a comprehensive theory differently from the manner pursued by atomism, still the following points must be admitted:

(1) This theory can never be a mere inventory, in the sense of having a definite symbol attached to each individual fact: it would in such a case be as difficult to command the situation as it would be to have experience of all the facts individually and severally. Like the present atomism, therefore, it can be nothing more than a body of directions for the construction of a theory and view of the world.

(2) If we will but free ourselves from illusion regarding the meaning of a differential equation, or of any continuously extended magnitude generally, we cannot fail to see that the phenomenologic view of the world is itself necessarily an atomistic view, viz., a direction to think by certain definite laws the time-changes of an immensely large number of things arranged in a manifold of probably three dimensions. The things in question may of course be either homogeneous or heterogeneous, variable or invariable. By assuming a large finite number or the limit of a continuously increasing number, our construct could correctly represent all the phenomena.

Assuming the possibility of an all-embracing theory of the world, every feature of which has the same evidence as Fourier's theory of the conduction of heat, it is still a question whether it can be more easily reached by the phenomenologic method or through the perfection and empiric confirmation of the constructs of atomism. It would be even permissible to assume that several representations of the world were possible, all of which possessed the ideal traits.

L. BOLTZMANN.

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